

# MADROÑO

A WEST AMERICAN JOURNAL  
OF BOTANY



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# MADROÑO

## A WEST AMERICAN JOURNAL OF BOTANY

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## REALITY, EXISTENCE, AND CLASSIFICATION: A DISCUSSION OF THE SPECIES PROBLEM

BENJAMIN H. BURMA

### INTRODUCTION

In 1949 the writer published a short paper setting forth certain views concerning the species concept, which elicited several other papers on the subject. One of these (Gregg, 1950) observed that writers on the subject of the species concept had in general been guilty of lack of clarity in the formulation of the ideas and definitions involved. The writer fully agrees with this view, and the present paper may be regarded as an attempt to survey, in a relatively short space, the basic philosophical assumptions and definitions involved in this phase of biology, and their application to the subject at hand. This has not proved a simple task and I doubt that the aim of clarity has always been achieved. On the other hand, a number of the concepts involved are rather difficult, and often I have had to choose between greater clarity or brevity, and had to choose brevity.

It may be well to point out at once that the ensuing discussion will, at different times, proceed on different levels. (On one level of discourse it is perfectly legitimate to refer to a table top as solid. In a discourse on the atomic level, such an assertion would be absurd.) The different levels have not in general been labeled, but every effort has been made to avoid confusion of the level of discourse. In the more strictly biological part of this paper, it will be apparent that the general level is that of a "picture-of-Nature," in which phenomena are interpreted in terms of "models." The physical sciences, and particularly physics, have gone beyond this level, and eventually biology will also, I am quite certain. If the state of the science were sufficiently advanced, it seems very likely that the discussion in the last part of this paper might well have been in terms of open energy systems in a field of energies, in something of an analogue of a physical field theory. For the present, however, we must be satisfied with less precise methods of dealing with the phenomena in question.

Several people, all members of the staff of the University of Nebraska, have been kind enough to read the entire manuscript and offer helpful criticism. For this I am indebted to Dr. Dwight D. Miller, of the Zoology department; Dr. Adam Skapski, Physics department, and Drs. William H. Werkmeister, Thomas F. Storer, and Bruce Waters, Philosophy department. I am particularly indebted to Dr. Storer, especially for his advice concerning the use of existence as a predicate. Nathan Mohler assisted in the preparation of the illustrations.

### STATEMENT OF THE PROBLEM

The question before us may be stated as "Are species objectively real units existing in the real world?" This innocent seeming question has in

it several words which are exceptionally difficult of definition and which are notoriously used by different persons in different ways. Our first task, then, shall be to attempt to definite, or at least describe the usage, of certain words or phrases in the above question in such a way that there will be a minimum of uncertainty in the mind of the reader as to the manner in which each is used in this paper.

It may be well first to say a word concerning verbal definition as such. It must be realized that any definition attempted can never be anything except the beginning of an infinite regress, at least within the limits of the language used in the definition. Thus we might define "beer bottle" as "brown glass container." This, of course, merely substitutes three undefined words for two. Each of these must then be defined, then each of the words in this definition and so on, theoretically without limit, hence an infinite regress. Since the language has a finite number of words, however, the regress will not actually be without limit as to *new* words introduced. A point will be reached where the available words are exhausted and new definitions will involve only reused words. Thus, in actual practice, we will be faced by circular definitions of the type of "Feature means peculiarity, peculiarity means characteristic, characteristic means feature." In any case, the essential point is that, in the last analysis, there is no such thing as a final definition, such being impossible unless one wishes to assume the existence of terms whose definitions exist in-the-nature-of-things. The mystic or theologian may have such but the scientist does not. In the problem at hand, then, definitions will be stopped at least this side of the point of diminishing returns.

#### MEANING OF "REAL"

Our first task will be to specify the meaning to be ascribed to the word "real," as it will be used in this paper. The word is used in so many senses that it is of first importance to make certain that its usage is always as clear as possible.

In this paper the word will be taken in its usual, or what I take to be its usual, "scientific" sense. All scientific inquiry has as a basis some postulate, or postulates, concerning reality and the relation of perceptual data to this reality. The following postulates and definitions are those on which this paper is based, and, I believe, they would be very widely recognized as being usual postulates. They are:

(1) There is a physical reality. (This is an unprovable assumption, but whether true or not, all the sciences proceed *as if* it were a fact and not an assumption. Sensory data may also consistently be explained on the basis of such a postulate as — The total reality is mind, and matter is a non-physical construct of the mind, that is, that reality is non-physical. Both postulates have been the basis of philosophic systems concerning the universe, and probably with equal success. Why then should we postulate a physical reality? There are a variety of reasons, some logical and

some not. The non-logical reasons are certainly of great force and are expressed in such statements as "If there were no physical reality, then science would be impossible," or "I would see no reason to study the sciences." Such attitudes place a powerful bias in minds of the type that would be interested in the physical and biological sciences in favor of the postulate of a physical reality. In addition, however, there are more logical reasons. If there is more than one mind in the universe, then, the argument runs, is it not strange that two minds would reach agreement regarding some "physical" fact in the absence of some underlying physical reality. It is easy to argue against this view, but to some minds, mine among them, such arguments are unconvincing.)

(2) There is some sort of relation between perceptual data and total reality. (This, again, is unprovable. Perceptual data may in themselves be reality—Berkelian idealism and similar epistemologies. However, if there *is* a physical reality, it would be completely outside the range of any knowledge unless there were a relation of some sort between perception and total reality.)

(3) Perceptual data give us our only first-hand data of the real world. (This is unfortunate truth, rather than assumption. It is merely the universally acknowledged fact that, for example, sight is a subjective, not an objective experience.)

(4) There is some sort of a *systematic, regular* relation between perceptual data and total reality. (If relation between the two were haphazard and unsystematic, it hardly seems likely that there would be any orderliness to the world of the senses. Such a postulate, though again unprovable, is probably a necessary basis for any theory of communication.)

(5) The real world, as the term will hereafter be used, is the reflection of total reality in perceptual data, and such that two or more observers may reach agreement in correlating their individual perceptual data. (This is, of course, a definition rather than a postulate. It is simply a more precise statement of the generally accepted idea that anything real will be perceptible to two or more observers in such a fashion that mutual agreement may be reached concerning the nature of the object. Note that the definition specifically excludes the perceptual data of the insane, the "abnormal," and the mystic. This does not deny the validity of these percepts, but any reality involved falls outside the "real world" of science.)

(6) Since total reality is non-perceivable, data received by our senses give us our only knowledge of the real world of total reality, and, for the individual observer, constitutes the real world. [This final postulate is, of course, the basis of relativism, whether in physics, philosophy, or elsewhere. The first known statement of this principle was made about 450 B.C. when Protagoras said that "Man is the measure of all things." It means that the first (logically) task of any observer is to discover the

rules for transforming the data from his observational system (physical, not notational) into that of another observer, and vice versa. This is actually a necessary precondition to point (5) preceding. The appreciation and successful solution of this problem for systems in non-linear relative motion is one of the greatest accomplishments of relativistic physics. Although it is not generally appreciated, the biological sciences are not, by some divine fiat, exempted from these necessities.]

(7) In addition to the above six postulates and definitions, I would add the following — that only is real which possesses extension in space-time. This is to say that reality is at least four-, not three-dimensional. The neontologists have consistently treated the species problem as though the organisms involved had only extension in space and not in time, the usual reason given being that “although paleontologists may have to deal with time in connection with organisms neontologists deal with such short spans of time, comparatively, that the time factor may safely be ignored as of no importance.” However, no one espousing this view has *ever* given any reason why one should ascribe reality to the three-dimensional shadow-pictures they describe or why the objects of their study should be exempted from the canons of reality applying to the other physical and biological sciences. Until such convincing reasons are given, and I know they will not be, we are justified in rejecting as entirely unscientific any views on the species-problem which are not discussed in terms of the space-time continuum.

#### THE MEANING OF “EXISTS”

The word “exist” has also been fruitful of misunderstanding. To say that a thing exists is to say that it is not fictitious, but an actuality, and since actuality embodies acts or events in the real, physical world, the world of things, it embodies a set of characteristics peculiar to that thing. One of these characteristics is the extension in the space-time continuum. That which does not have such extension cannot be said to exist in the sense used in our statement of the problem.

The use of “exists” is important in a consideration of Gregg’s paper, “Taxonomy, language and reality” (Gregg, 1950). Since the difficulties in which Gregg finds himself in this paper are not uncommon, it may be useful to analyze certain of his arguments in some detail (p. 421 *et seq.*).

First, he examines the proposition “All species exist,” a proposition of the type “all fish swim,” which may be recast as “for any  $x$ : if  $x$  is a fish, then  $x$  can swim.” A similar recasting of the first proposition gives “for every  $A$ : if  $A$  is a species, then  $A$  exists.” The contrary of this is then given as “not (for every  $A$ : if  $A$  is a species, then  $A$  exists.)” This, Gregg says is inconsistent as implying the contradictory statement “there exists an  $A$  such that  $A$  does not exist.” I cannot agree with this. It is true that the last statement is superficially, at least, contradictory, but I do not regard it as legitimately following the contrary. The contrary statement may be

transcribed in ordinary language as "It is not true that for every A, if A is a species, then A exists." This statement becomes contradictory *only* if the "is" is taken itself to mean "exists," which does not follow.

A similar difficulty is involved in his analysis of the proposition "some species exist" which he inferentially, by example but not directly, recasts as "there exists an A, such that A is a species, and A exists." Here again, "exists," the first one, is improperly substituted for "is." The difficulty lies in the failure to distinguish between what we may call logical existence and real existence. The meaning of the two terms may be illustrated as follows: Consider the class of unicorns. We erect the class definition — unicorns are those animals with the head, neck and body of a horse, chin tuft of a goat, legs of a buck, tail of a lion and a long straight horn on the middle of the forehead. This class is properly constructed; it may be used in syllogisms, etc., and may be said to have logical existence. On the other hand, no such animal ever lived, there is no actual animal of the real world meeting this description, and for this reason, the individual unicorn is said to be without real existence in the sense defined in the first part of this section.

If "exist" of the propositions "All species exist" and "Some species exist" is of the type of logical existence only, then the discussion of these topics is *purely* theoretical and of no possible application to the discussion in this paper and in my former paper (Burma, 1949) which is concerned only with real existence, nor is it of any particular interest to biologists as biologists, whose concern is also with real, not logical existences. On the other hand, if "exist" in the two propositions refers to real existence, or has any flavor of it, then Gregg's conclusion that both propositions are necessarily true is certainly false since the statements he cites, "There exists an A such that A does not exist" and "there exists an A, such that A is a species and A exists" involves contradictions *only* if it is not realized that the first "exists" refers to logical existence and the second to real existence. For this reason, the logical contraries of the two original propositions: "It is not true that for every A, if A is a species, A exists," and "There is an A such that A is a species and A does not exist," where *only* the words "exist" refer to real existence, contain no internal contradictions at all, exactly opposite the conclusion of Gregg.

When (p. 423) Gregg says that the statement " 'There exists no A such that A is a species' [better stated as 'There is no A such that A is a species and A exists'] is false for there are species: *Homo sapiens* is one, *Escherichia coli* is another, and *Anopheles quadrimaculatus* is another", he is in error in his reasoning. Let us recast this statement of Gregg's in syllogistic form:

The taxonomic unit *Homo sapiens* exists  
*Homo sapiens* is a species  
 Therefore, species exist

Put thus baldly, it is apparent that we are again faced with a confusion in

the usage of "exists." If, in this syllogism, "exists" refers only to logical existence, we need not quarrel with Gregg but the syllogism then has nothing to do with our problem.<sup>1</sup> The *apparent* proving of the (real) existence of species arises when the first "exists" refers to logical, and the second to real existence. However, since the two "exists" are actually different words which happen to be spelled the same, the conclusion "Therefore species exist" does not follow from the major and minor premises. If both "exists" are taken to refer to real existence, nothing is actually proved since the major premise then *assumes* the very point we are supposedly out to establish! In short, this apparently convincing argument is *sophistical* and false. (Parenthetically, it may be pointed out that since the above arguments are basic to Gregg's thesis, we must conclude that his thesis is not proven.)

No one can deny that the units of the taxonomic system have logical existence, so does a mathematical line. Our problem concerns the real existence of species and Gregg's paper does not help us in this problem. Let us now consider some aspects of the general problem of classification.

#### THE PROCESS OF CLASSIFICATION

Classification, as a process, is a fundamental necessity in human life. We are presented from birth with a bewildering variety of sensory images, and the infant's first task is to bring some sort of order out of this chaos. Among the first of these orderings is the recognition that one particular group of sensory images belongs to a class that provides food and comfort. The first vocalization of the infant often names this class — *mama* —, in this case a class of one member. By the time the child is a few months old, the class, *mama*, is firmly identified. The very development of skill in vocalization depends on and develops with this ability to classify. The infant learns that the word "dog" is associated with a certain group of sensory images, and that when he points to a member of this class, and says "doggie," he will be rewarded.

Indeed, we can see that this ability to classify is necessary to the ability to communicate. Nouns and adjectives, our chief classifiers of the world about us, are an absolute necessity for the exchange of information. Consider the number of sensory images, inherent or implicit, in the noun "Man." Consider the time saved by classifying this mass of images in the one word. Indeed the very process of vocalization is one of classification, for when I say, "men," I have placed a certain group of sensory and conceptual data in the class "men." Because classification is necessary for communication, it does not follow that: (1) precision is gained thereby, or (2) that classification is a feature of the world itself rather than our own mental processes.

<sup>1</sup> Actually, the "syllogism," and the argument so formalized, have certain peculiarities which, apart from other considerations, render the validity of the argument doubtful.

The first usable formalization of classification must be credited to Plato and Aristotle. The "archetype" of Plato may be thought of as a spiritual prototype of the objects of the material world. (Plato's *εἶδος* which is almost invariably translated as "idea" I have here rendered as "archetype." "Idea" inevitably, and especially for non-philosophers, carries with it a connotation of non-materiality. However, the *εἶδος* of Plato was the very essence of reality and in every sense material. The world of matter was, in fact, considered to be only a pale reflection of the world of the *εἶδος*.) Thus, there was an "archetype" of "dog." Matter, by partaking of this "archetype" became a dog. Thus the process of classification became merely a process of recognizing the "dogginess" inherent in any dog. (Observe the essential identity of this process with the process of recognizing a species by means of its essential characters.) Similarly, one recognizes the class of bears by recognizing the inherent "bearness" of certain animals. Thus the physical world was considered to be neatly compartmented into classes corresponding to "archetypes" and the process of classifying consisted only in the more or less intuitive recognition by the philosopher of the indestructible "archetype" behind the shifting veil of the physical world.

Aristotle's views were basically quite different although in practice the results were about the same as with Plato's system. According to Aristotle, every material object is the result of the union of two principles, matter (material) and form (non-material). Matter is regarded by him as possessing the capacity for form or being potentially formed matter. However, form has being only insofar as it is expressed in material objects. Thus the dog is a "dog" because the matter of which the animal is composed had the potentiality, the form, of "dog." From this point classification proceeds by recognizing "forms" as we before recognized "archetypes."

This general view of classificatory process was regarded, during revival of Aristotelianism, as being compatible with Christian dogma, and as such passed into western philosophy, either explicitly or implicitly. Linnaeus' classification was almost certainly Aristotelian in its basis, yet it is equally certain that in practice he leaned strongly to the Platonic view. Thus in his *Philosophia Botanica* he states "species tot numeramus quot diversae formae in principio sunt creatae" (freely rendered as "our classification contains just as many species as there were different forms originally created"), an unexceptionably Aristotelian statement. However, in his *Classes Plantarum* he states "species tot sunt quot diversas formas ab initio produxit infinitum Ens" ("there are as many species as there were different forms produced in the beginning by the Infinite Being"). Here it is quite plain that his species are archetypal, fixed and immutable from the day of creation.

Such a mixed Platono-Aristotelian view of the species generally lurks in the background of classifications even today. Thus, when the native Papuan (Mayr, 1949, p. 371) recognized a given individual bird as a fan-

tail, he compares this individual with a composite mental image of all fantails of his experience, checks to see that the individual in question conforms in essential characters, and, if it does, announces, "This is a fantail." In so doing, the Papuan is a reasonably accurate facsimile of a Platonic philosopher. He extracts from the *changing* appearances of ever-changing matter, the eternal "archetype" of "fantail" — the veriest essence of "fantailness." One might say, by definition, that any individual conformation of matter partaking strongly of the "archetype" of fantail *is* a fantail. Avowed Platonists are rare in the world today; unavowed, unconsciously Platonic Platonists are indeed legion, perhaps to the extent of including all mankind.

Such a process of abstraction, identification, is the basis of classification. The chief danger is in the unconscious use of "archetype" as the basis of classification, a piece of mysticism productive of many difficulties, not the least of which is the according of the word "species" with a connotation of "If I recognize A as a species, I must be recognizing something, and that something is real, otherwise I wouldn't recognize it, therefore species A is a reality," the reality in the last analysis being the "archetype" of species A.

#### SOURCES OF AMBIGUITY IN CLASSIFICATION

One difficulty with almost any scheme of classification is that, sooner or later, uncertainty arises as to whether some particular individual should or should not be referred to a particular class. Such ambiguous cases may arise from two rather distinct, though sometimes not necessarily distinguishable causes. We may refer to them as extrinsic and intrinsic ambiguities respectively.

Extrinsic ambiguities are those external to the objects being classified and inherent in the *class* involved. They are essentially difficulties in the *definition* of the class. For example, it is difficult to state exactly whether or not viruses belong to the class of living things. This ambiguity is primarily due to the lack of certainty about the definition of *life* and not about the pertinent characteristics of the viruses.

Intrinsic ambiguities exist in the individuals being classified rather than in the definition of the class involved. These exist primarily where the individual is a member of a continuously variable series and a class is set up for a portion of this series. In this case, the *definition* of the class may be perfectly unambiguous, but uncertainty will of necessity exist regarding the assignment of individuals on the border of the defined class. Such ambiguity is inherent in the situation.

#### THE INDIVIDUAL

The basic unit of a biological taxonomic system is the individual. The individual may be made of cells, organelles, etc., etc., and these constituent structures will be important in understanding the individual, but they do

not in themselves take part in the taxonomic hierarchy. Let us first, then, seek an understanding of *what* an individual is.

To avoid unnecessary complications, we may restrict our discussion to non-colonial organisms. Introduction of such ancillary problems would lead us far from our present purpose. Biologically the individual is important as the unit through which life maintains itself in space-time. It is the smallest unit which can, either by itself, or through cooperation with another unit, ensure that a given kind of life persists from generation to generation. As such, an essential function is the passing on of hereditary factors so that a genetic continuum is maintained from ancestor to descendant. The simplest way to consider this aspect, and others, of the individual, is by reference to the "world line" of the individual. ("World line," as will be seen below, refers not to the fully relativistic world line of Minkowski, but to a simplified version useful to our purpose. Specifically, we will use as coordinates  $x, y, z, t$  rather than  $x, y, z, ict$ .".)

Since the concept of the world line may be unfamiliar to some readers, a short explanation is in order. The world line of an object is the path that an object takes in space-time. If one wishes to plot such a world line, four coordinate axes are necessary, three spatial and one temporal. (For a good, reasonably non-technical, discussion of Minkowski's world line, see d'Abro, pp. 195-200, 1950.) Considering the difficulties involved in drawing a tesseract on a sheet of paper, we need not attempt to plot a line, or volume, in one. Again, for the sake of simplicity, we may adopt one axis for the space coordinates combined and a second for the time coordinate, as in Figure 1. For our examples we shall plot the world lines of individuals as lines. Thus Figure 1 shows the world line of a given individual from time  $T_1$  to  $T_2$ .

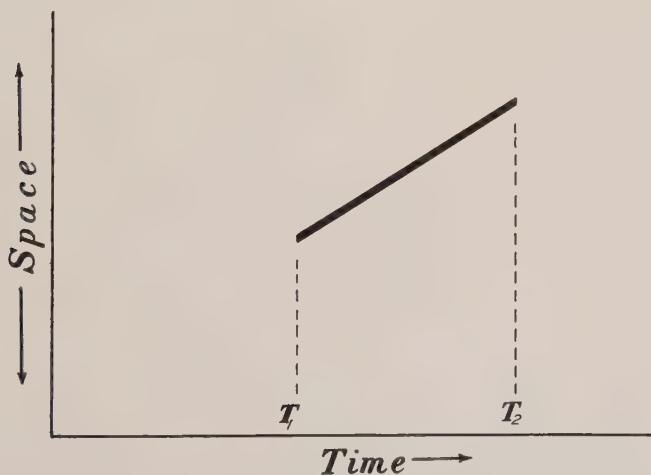


FIG. 1. World line of an individual in space-time during time-segment  $T_1$   $T_2$ .

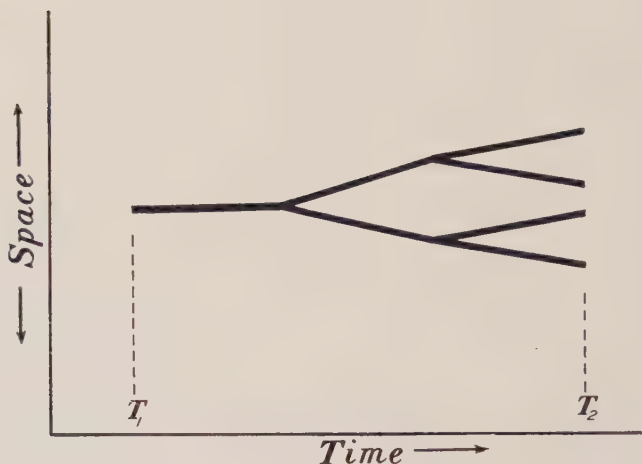


FIG. 2. World line of an asexually reproducing individual with two episodes of reproduction by binary fission.

If our organism reproduces by fission we may, starting at time  $T_1$  show it and its descendants as in Figure 2. Notice that we have reached a point where the unambiguous designation of an individual in terms of its world line becomes difficult, and for much the same reason that we have difficulty in precisely designating individuals in highly colonial animals; that, viewed over-all, we have continuity, and it is only at specified time segments that individuals have their world lines sharply differentiated. (The ordinary view of bodies in space-time is that they are bodies with three spatial dimensions traveling through space-time. In this view, the line of Figure 1, and 2, is actually a composite of an infinite number of points so that the continuity shown in Figure 2 is effectively real but not physically real. An alternative view is that objects are actually four dimensional, although for some reason our preception only reveals three-dimensional cross sections. In this view, the continuity would be in the highest degree physically real. I know of no exploration that has been made of this extremely interesting alternative view, although it has a number of intriguing consequences.) [While this paper was in manuscript, Dr. Werkmeister called my attention to a paper (Williams, 1951) in which one aspect of this interesting view is examined.]

Sexually reproducing organisms may similarly be shown by world line plots. In Figure 3, where such an example is given, sex cells are shown arising from the parents, fusing, and giving rise to new individuals. Note that this monogamous couple has produced two offspring. In the case of such sexually reproducing diploid organisms it is somewhat easier to designate the individual, but difficulties remain. The individual may be said to come into existence when the chromosome sets of the haploid sex cells combine. Even this definition is less precise than it seems. What is im-

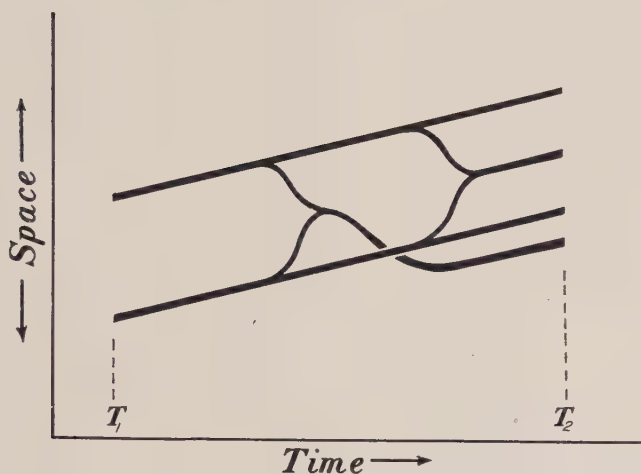


FIG. 3. World lines of two sexually reproducing individuals with formations of gametes, zygote, and new individuals.

portant is not the definable separateness of the individual but the continuity between parent and offspring, i.e., the continuity of germ plasm—and of cytoplasm, too, if cytoplasmic inheritance is of importance.

#### THE BREEDING POPULATION

Although we have been unable thus far to say precisely where, in space-time, one individual begins and another leaves off, let us proceed to a consideration of the next larger aggregation of organic units. In sexually reproducing organisms, any given individual will ordinarily be within the range of activity of a considerable number of individuals of the opposite sex who could, if the opportunity arose, mate with this first given individual and produce viable offspring. Such a group, composed of one individual together with its immediately potential mates, we may refer to as first order breeding populations. Obviously, first order breeding populations will be highly unstable, rapidly changing in composition in space-time, and are more valuable as analytic units than anything else. By definition, there are in the world as many first order breeding populations as there are sexually mature individuals, and each such individual might be a member of several thousand first order breeding populations.

Within a first order breeding population, gene flow is, by definition, actually or potentially complete and free. If we survey all the first order breeding populations to which a given individual belongs, and pass from these to other connected breeding populations, we will find certain geographic areas within which gene flow is actually or potentially as free and complete as within a single first order population. However, sooner or later we will come to a boundary, geographic or some other kind, across which gene flow is restricted. The nature of the barrier to free gene flow

will, of course, depend on the organisms involved. For land snails, a small river might well constitute such a barrier; the same stream would be no barrier whatever to most birds. The aggregate of first order breeding populations within which gene flow is relatively free and unrestricted and between which there is some restriction of gene flow, we may call second order breeding populations. Here again we are dealing with a unit that is useful for purposes of analysis but which we would probably find was impossible to define *precisely* in nature.

Barriers between adjacent second order breeding populations will often be temporary in character. When such barriers are removed, the second order populations involved will, of course, merge and become one. Let us now examine the world lines involved in these second order populations.

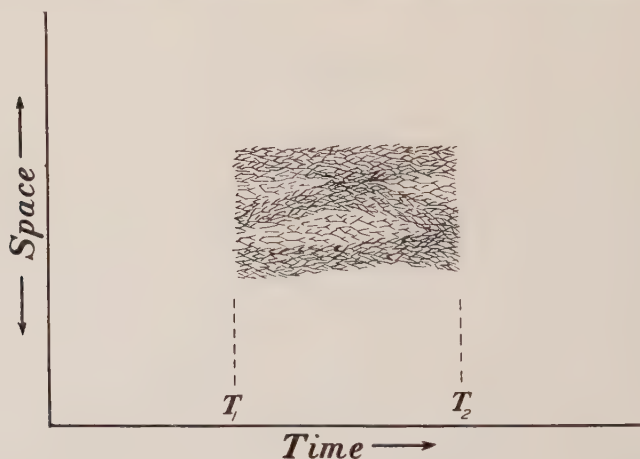


FIG. 4. Temporary separations and fusions of related second order breeding populations in space-time. (See text.)

In Figure 4, which is highly simplified, individuals are shown as lines. Because genetic separation of these second order populations is incomplete and impermanent, such second order populations will not maintain any significant genetic differences over any period of time, and as a consequence morphological differences between the populations involved will be minor and transitory. If two such complexes of second order populations become separated to the extent that gene flow is markedly restricted and over such a length of time that barriers to gene flow, inherent in the animals themselves, develop, gene flow will still be restricted when contact between populations is renewed, or may even be actually or essentially nil. Such groups of second order populations we may refer to as third order breeding populations. A third order of populations will tend to evolve independently of other such populations since exchange of genic material will be restricted wholly or mostly to individuals within the population.

With the third order breeding populations we reach the end of our natural hierarchy. All such populations which are in themselves unable to exchange genic materials will be different. Any further building of a hierarchy will be based on decisions as to *amount* of differences between these populations. Such a decision is a *value* judgment, and is not part of the real world. Third order breeding populations are *different*. Only the human mind makes an issue of *how* different they are.

Note that the foregoing analysis is essentially independent of the mechanism by which the hereditary principle is passed from parent to descendant. In the discussion we have, for convenience, spoken in terms of the gene theory of inheritance, but this is *only* a convenience and in no wise necessary to the argument. Only two major assumptions are made in the foregoing analysis — (1) that offspring resemble parents because of some physical “principle” which they receive from their parents, and (2) that, in the course of the passage of this hereditary principle from one generation to the next, hereditary differences may arise and be passed on to further offspring. I do not believe that any reputable biologist would quarrel with either of the above assumptions today. They are among the most ancient of bases of evolutionary biology. Anaximander and Empedocles, nearly 2,500 years ago were familiar with these tenets. Note also that no special theory of evolution is here assumed, only evolution in its most general sense.

#### BIOLOGICAL TAXONOMY

We may now inquire as to where, in the preceding hierarchy of breeding populations, the usual taxonomic hierarchy fits. It is apparent, I believe, that all rest within what we have just defined as third order breeding populations except for subspecies. Subspecies seem to be second order breeding populations, if they have real existence at all, a matter we shall discuss below. According to the usual definitions, subspecies, in general, interbreed more or less freely if allowed to mix in nature and will, with such continued opportunity, become indistinguishable. All the other categories must then be third order populations between which gene flow is restricted or absent.

Let us then examine the species to see if any reality may be ascribed this supposed biological or taxonomic unit. First of all, be it clearly understood that the *logical existence* of species as a taxonomic unit is not in question. Whatever definition one may wish to use, species will be a defined class of some sort, and as such may be said, no more and no less, to exist in precisely the same sense that the class of unicorns may be said to exist. But, as I have intimated before, this is not our problem, which is — “do species have real existence, and if so, in what manner.”

Classes, as such, have no real existence. They are, as we have seen, mental constructs and as such lack actuality in the sense here defined. No one ever saw a mental construct walking down the road. Species, *as*

*classes*, thus are obviously without real existence. To construe the actuality and the existence of classes thus rigidly, however, may seem overly severe, and might be said to beg the question insofar as our central problem is concerned. In what sense at all can any class be said to have real existence?

Since we have seen that classes cannot be said to have real existence in actuality, any tincture of reality ascribed to them must be *analogical* and comparable to the reality of the individuals making up this class. As a neutral example, let us examine the class of "tables." We may define a table as a manufactured object consisting of an essentially flat top supported by one or several legs or pillars. (Note that this is a structural, not a functional definition.) This definition is quite unambiguous and any residual ambiguity will be largely extrinsic, rather than intrinsic, as defined above. (It must be understood that this applies primarily to structural definitions. I would hesitate to estimate how many problems needlessly plague biologists because of their fondness for functional rather than structural definitions.) What canons must we now specify in order that we may ascribe some aspect of real existence to this class. I would specify the following: (1) the class definition must be essentially unambiguous and any residual ambiguities must be extrinsic and not intrinsic; (2) the class must have at least one member which has real existence; (3) the member or members of the class must have a demarcable boundary, at least theoretically, in space-time. I will now discuss these specifications in order.

(1) The class definition must be essentially unambiguous, and any residual ambiguity must be extrinsic and not intrinsic. Members of a class may be designated in one of two ways. They may be designated by individual specification. For example, I may say, and touch with my finger, "this desk, this chair and this bookcase constitute the class of whingdings." Such denumerable, individually specifiable classes are a distinct minority insofar as practicability is concerned. To so specify the class of dollar bills, for example, would be not only wearisome but downright impossible if one wished to include those bills of the past which have since been destroyed. Thus the commoner method of designation of the members of a class is to describe the characteristics an individual must have to be considered a member of that particular class. The specification above, concerning unambiguity of definition, is necessary to fulfill the fifth canon of reality discussed in the section "Meaning of 'real' " above, which, in essence, states that reality must be communicable. Any ambiguity of definition, and particularly an intrinsic ambiguity, will result in a failure of communication. If Smith cannot describe a given phenomenon with essential unambiguity to Brown, Brown will never know whether or not he made observations comparable to Smith's. Yet such agreement is necessary before reality may be ascribed to the phenomenon. An ambiguously described phenomenon may be real, theoretically, but in actuality, no one

can prove it and it is thus effectively without reality. Since the definition of "table" is essentially unambiguous, or can be made so, it has satisfied the first canon.

(2) The class must have at least one member that has real existence. This simply says that reality can be ascribed only to real things. Thus the class of unicorns *might* possibly be unambiguously defined, but since there are no unicorns and never have been, it is an empty class and has no tincture of reality as defined above. Not all cases are so simple, however. The class of man-carrying space-ships is today an empty class, and so one without reality. I would hate to have to bet very much that it would be an empty class, say, five years from now. Since we do not know the future, as a practical matter we must understand that the one member necessary to ascribe reality to a class may be of past or present existence, with all bets off where the future is concerned. Similarly, I would hesitate to stake my life on the contention that there are not unicorns somewhere in the universe. Here again, as a practical matter, we must restrict ourselves to things within the possible present knowledge of man. In any case, the class of "tables" has at least one member, so that it fulfills this canon also.

(3) The member or members of the class must have a demarcable boundary, at least theoretically, in space-time. This specification is closely linked with the preceding ones. What it says is that a "real" class must in some sense be a naturally existing unit. Let us test the class of "tables" in this respect. At some time and place in the past, the first table was manufactured. The world line of this individual table could be plotted from its inception to its dissolution. Similarly world lines for all subsequent tables could be plotted. And similarly, sometime in the future, the world line of the last table will end. Thus we might draw a four-dimensional boundary around the entire class of "tables." This means that it is in a very real sense a natural unit strictly analogous to an individual. Since the real existence of a class is only by analogy to real existence of an individual, such a class may be said to have this analogical reality. Observe that such a unit is possible only if the class is unambiguously defined.

So we see that the class of "tables" fulfills all three canons and we are now justified in saying that the class of "tables" has real existence.

We may now ask if a biological species, any one, fulfills these three canons. It probably fulfills the second one, since if it is a "valid" species it has or had real individuals for members. The other two canons are a different matter, however. Consider Figure 5, in which the zone of short lines is a diagrammatic representation of the world line of the individuals making up a third order breeding population, a single phyletic line. At times *a* and *b* there existed two populations, which, we shall assume, were different enough to be considered two "species," *a* and *b* respectively, by ordinary standards. We may also assume, for the sake of simplicity, that no other species would be recognized in the interval between times *a* and *b*. Can *a* and *b* be unambiguously defined in such a way that individuals of

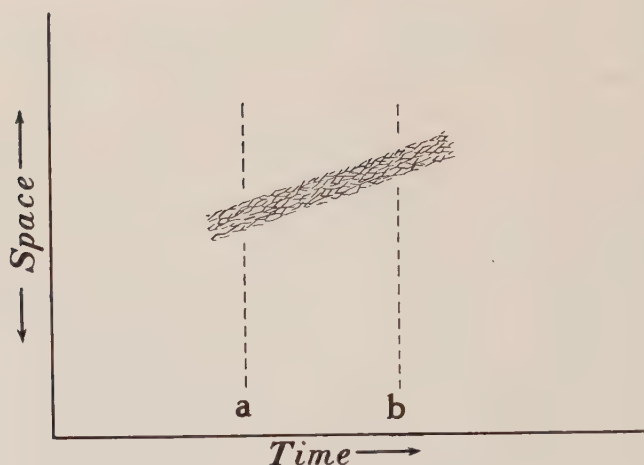


FIG. 5. Time segment of a third order breeding population.

the time interval  $a\ b$  can be unambiguously distributed between  $a$  and  $b$  and so that  $a$  and  $b$  will be demarcable units? It must first of all be observed that  $a$  and  $b$  are components of a biological continuum. (The succeeding discussion is based on the assumption that evolution on the "species" level took place in minute, usually unmeasurable, stages. I know of a great amount of actual evidence from the fossil record that this is true. I know of *no* evidence that it is not usually true. "Species formation" in the plants by the processes of hybridization and polyploidy are not considered. These processes would require the recasting of details of the argument of this paper insofar as it affects plants, but not animals. The general line of argument is not affected.) If we have an intracharacter continuum, there are obviously, even by definition, no natural demarcable units within it. Any definition attempted will be *intrinsically* ambiguous. Thus species as classes do *not* fulfill the first and third canons and cannot be said to have real existence in any sense.

We may now ask, if species as classes are without real existence, what have biologists been talking about all these years? Judging by descriptions of individual species, all too many did not know what kind of a unit they were talking about. However much lip-service has been paid by biologists to the dynamic philosophy of evolution with its Heraclitean flux, almost all biologists have dealt with species in terms of the outmoded, static, Platono-Aristotelian philosophy. In discussions of evolution, the following type of phrase is commonplace — "species  $a$  evolved into species  $b$ ." This, *as it stands*, means that there was some sort of a definable unit, species  $a$ , which changed without any particular intermediate phase into species  $b$ . Such one-step speciation is possible in plants by hybridization or polyploidy. In animals, it is apparently rare and unimportant. In any case, the author of the phrase would probably say that

he had no such thing in mind, that, of course, he did not intend to say there was any such jump from *a* to *b*, but he would go right on talking in these terms, and, linguistics being what they are, those who talk in such a fashion are almost certain to think in these same terms. Anyone who looks through evolutionary literature with this in mind can quickly locate literally hundreds of examples where it is obvious in context that the writer was thinking of species as discrete, objective, real units, regardless of any protestations the author may have that he realized the subjective character of biological classification. What all these workers, neontologists and paleontologists, have actually been dealing with is an arbitrarily set-off segment of a continuous phyletic line. For those who say, "Well, of course, but what of it?", I can only reply that it is extremely improbable that anyone who *thinks* in terms of the static species concept fails to appreciate the full implications of a dynamic philosophy such as evolution. It would be easy to quote examples to support this contention, but I prefer not to do so.

In this situation, the most obvious thing to do is to discard such a term as "species" with its overpowering load of undesirable connotations, and use or coin a new word for these phyletic-line segments which have been hitherto called "species." I do not think any such course will be adopted, the International Rules of Zoological Nomenclature being one of several reasons. To minimize these effects, however, I would suggest the following steps. First, all taxonomic units of whatever kind should be recognized explicitly for what they are—arbitrarily erected, man-made constructs, incautious use of which may result in obscuration to a degree which outweighs the convenience of recognizing such units. Secondly, the idea of the reality of evolving populations of individuals should always be kept in the forefront of any discussion of the evolutionary process. Such highly abstract fictions as "species", in the conventional abstract sense, will only continue to obscure such discussions.

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DOCUMENTED CHROMOSOME NUMBERS OF PLANTS  
(See Madroño 9:257-258. 1948.)

SPECIES	NUMBER	COUNTED BY	COLLECTION	LOCALITY
<b>LILIACEAE</b>				
<i>Tulbaghia violacea</i> Harv.	n = 6	Gene Newcomb Univ. Calif.	Mrs. M. A. Schaeffer, UC†	Berkeley, California
<b>SCROPHULARIACEAE</b>				
<i>Castilleja lasseusensis</i> Eastw.	n = 12	G. W. Gillett Univ. Calif.	Gillett 289 UC	Shasta Co., California
<i>miniata</i> Dougl.	n = 24	G. W. Gillett Univ. Calif.	Gillett 292 UC	Shasta Co., California
<i>Paysonae</i> Eastw.	n = 12	G. W. Gillett Univ. Calif.	Gillett 283 UC	Shasta Co., California
<i>Wightii</i> Elmer × <i>affinis</i> Hook. & Arn.	n = 24	G. W. Gillett Univ. Calif.	<i>Bacigalupi</i> 4549 UC	San Mateo Co. California
<i>Wightii</i> Elmer	n = 12	G. W. Gillett Univ. Calif.	<i>Bacigalupi</i> 4557 UC	San Mateo Co. California
<b>COMPOSITAE</b>				
<i>Acanthospermum *australe</i> (Loefl.) Kunze	n = 10	S. Carlquist Univ. Calif.	Carlquist H9 UC	Kauai, T.H.
<i>Aster novae-angliae</i> L.	n = 10	C. J. Avers Ind. Univ.	C. J. Avers 6.IX.1952, IND.	Jefferson Co., Missouri
<i>patens</i> Ait.	n = 10	C. J. Avers Ind. Univ.	C. J. Avers 20.IX.1950, IND.	Lawrence Co., Indiana
<i>pilosus</i> Willd.	n = 24	C. J. Avers Ind. Univ.	C. B. Heiser 15.IX.1950, IND.	Monroe Co., Indiana
<i>prenanthoides</i> Muhl.	n = 9	C. J. Avers Ind. Univ.	H. T. Stinson 7.IX.1950, IND.	Huntingdon Co., Pennsylvania
<i>simplex</i> Willd.	n = 36	C. J. Avers Ind. Univ.	C. B. Heiser 20.IX.1952, IND.	Monroe Co., Indiana
<i>turbinellus</i> Lindl.	n = 48 — 50	C. J. Avers Ind. Univ.	E. Anderson 15.IX.1951, IND.	Jefferson Co., Missouri
<i>Chaenactis Douglasii</i> H. & A.	n = 12	G. W. Gillett Univ. Calif.	Gillett 332 UC	Shasta Co., California
<i>*nevadensis</i> Gray	n = 6	G. W. Gillett Univ. Calif.	Gillett 323 UC	Shasta Co., California
<i>Eatonella *Congdonii</i> Gray	n = 10	S. Carlquist Univ. Calif.	<i>Bacigalupi</i> 4368 UC	Kern Co. California
<i>Verbesina encelioides</i> (Cav.) Benth. & Hook.	n = 17	S. Carlquist Univ. Calif.	Carlquist H30 UC	Honolulu, Oahu, T.H.

\* Prepared slide available.

† Symbols used for herbaria are those listed by Lanjouw and Stafleu, Index Herbariorum, Part 1. The Herbaria of the World. Utrecht. Second Edition. 1954.

## CLARKIA EXILIS, A NEW CALIFORNIAN SPECIES

HARLAN LEWIS AND FRANK C. VASEK

Several new species of *Clarkia* have recently been described which have a very restricted distribution and which are morphologically very similar to well known and widely ranging species (Lewis and Lewis, Madroño 12: 33-39, 1953). These sympatric species pairs offer an unusual opportunity for the study of species differentiation. The new species described below forms another such pair with *C. unguiculata* Lindley (*C. elegans* Doug.). From herbarium studies, we had considered it to be a variant of *C. unguiculata*. However, when we observed the two taxa growing together in the Kern River Canyon, even a superficial examination indicated that the plants fell into two distinct groups with no intermediates. One of these was typical *C. unguiculata*, consisting of relatively coarse plants with conspicuously hairy buds that were not yet open. The other group consisted of slender, more delicate plants which were in flower. The flowers were similar to those usually found in *C. unguiculata*, although they were somewhat smaller and the petals were more slender. In addition, the buds of this group were not conspicuously hairy. Only two explanations were possible, namely, the two distinct groups were due to a simple genetic difference which was segregating into two classes with no intermediates, or the two groups represented distinct species. Present evidence indicates that the latter is the case.

*Clarkia exilis* sp. nov. Herba erecta altitudine ad 10 dm.; caulibus simplicibus vel ramosis, exilibus, glabris et glaucis; foliis superne lanceolatis, inferne angusto-ellipticis, 1-6 cm. longis, 2-14 mm. latis, denticulatis et glabris, sessilibus vel in basi ad petiolos usque ad 5 mm. longo angustatis; inflorescentium axe erecto; calycis tubo 1-3 mm. longo et annulo pilorum supra medium ornato; limbo 5-13 mm. longo, 1-2 mm. lato, sub anthesi connato et declinato; petalis unguiculatis, 5-15 mm. longis, 2-7 mm. latis; unguiculo gracili limbum aequante; limbo rhombiformi, roseo vel albo et in basi saepe roseo-purpureo maculato; staminibus 8, plerumque albis et similibus; stylo aequante staminibus; stigmatibus quadrifido, lobis rotundatis; ovario 8-costato, 6-16 (-20) mm. longo; capsula gracili 15-30 mm. longa, 2-3 mm. lata; semina 1 mm. longa.

An erect annual herb, 2-6 (-10) dm. tall; stems slender, simple or branched, glabrous and glaucous; leaf blades lanceolate above, narrowly elliptical below, 1-6 cm. long, 2-14 mm. broad, more than four times as long as broad, glabrous or essentially so, sessile or with petioles as much as 5 mm. long; rachis of the inflorescence erect, buds at first erect, then deflexed, becoming erect again as the flowers open; sepals oblanceolate, 5-13 mm. long, 1-2 mm. broad, green, remaining united and deflexed to one side at anthesis, minutely and sometimes sparsely puberulent; hypanthium obconical to campanulate, 1-3 mm. long, the ring of hairs at or above the middle; petals 5-15 mm. long, 2-7 mm. broad, divergent, con-

spicuously clawed, the claw slender, equalling the limb, the limb rhomboid, lavender-pink or white, often with a dark reddish-purple spot at the base; stamens 8, in two similar series, white, or the outer series sometimes reddish; pollen white or cream-colored, occasionally purplish-gray; style about the same length as the stamens; stigma with four rounded lobes; ovary 8-ribbed, bright green, 6–16 (–20) mm. long, puberulent, sessile or on pedicels up to 3 mm. long; capsule slender, curved, 15–30 mm. long, 2–3 mm. broad; seeds about 1 mm. long, tuberculate, slightly if at all crested.

Type. Kern River Canyon, one mile from the mouth, Kern County, California, *Lewis, Lewis, and Vasek 908*, April 19, 1952 (UCLA).

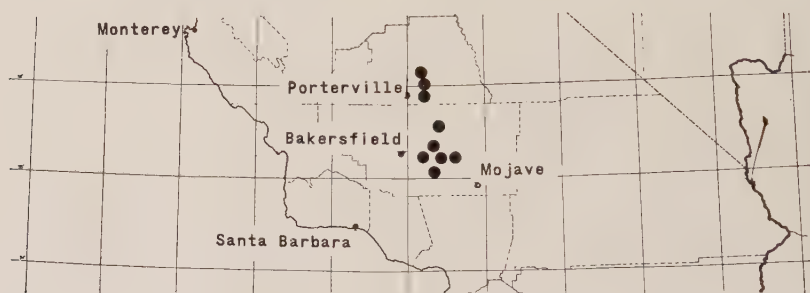


FIG. 1. Distribution of *Clarkia exilis* in Kern and Tulare counties, California.

Distribution. (See fig. 1.) Lower Kern River Canyon drainage, Kern County, north to lower Tule River drainage, Tulare County, California. Usually in shady or north-facing sites in the oak-digger pine woodland.

Specimens examined. Herbarium specimens have been examined at the following institutions: Pomona College, Rancho Santa Ana Botanical Garden, California Academy of Sciences, Stanford University, University of California at Berkeley and Los Angeles. We are grateful to the curators of these herbaria for the courtesies extended.

CALIFORNIA. Kern County: Kern River Canyon, *Abrams* in 1900; mouth of Kern Canyon, *Benson 3344, 3463, 3464*; lower Kern River Canyon, one mile from mouth, *Lewis, Lewis, and Vasek 908* (type); *Vasek 101, 121*; 2.3 miles from mouth, *Vasek 122*; 2.9 miles from mouth, *Vasek 123*; 3.8 miles from mouth, *Vasek 124*; 4 miles from mouth, *Abrams 11990, 12009*; 12 miles from mouth, *Keck and Stockwell 3264*; Rancheria Road, 9 miles from junction with Kern River Highway, *Vasek 125*; Breckenridge Mountain Road, 2 miles west of the junction with the road to the Kern River Highway, *Vasek 127*; 1.1 miles north of Granite Station on the road to Woody, *Vasek 130*. Tulare County: 3 miles east of White River, *Vasek 131*; Deer Creek Road, 11.4 miles west of California Hot Springs, *Vasek 132*; Tule Indian Reservation Road, 0.2 mile east of junction with Bartlett Park Road, *Vasek 133*.

The haploid chromosome number, 9, is based on examination of microsporocytes of three plants from the type locality and two plants from Rancheria Road (*Vasek 125*). Permanent slides and herbarium vouchers are on file in the herbarium of the University of California, Los Angeles.

Relationship. *Clarkia exilis* is morphologically very similar to *C. unguiculata* and is undoubtedly most closely related to this species. The hybrid between them is difficult to produce in the garden, however, and the two species remain distinct in the field although they frequently occur in mixed colonies throughout the known range of *C. exilis*. Where the two species grow together our field observations indicate that they are easily distinguished on the basis of habit as well as the long spreading pubescence on the ovary and calyx of *C. unguiculata*. However, variants of *C. unguiculata* without conspicuous hairs on the ovary and calyx are found in other areas. No single character distinguishes all of the variants of *C. unguiculata* from *C. exilis*, but in general the relative length of the ovary and calyx at anthesis is definitive. The length of the ovary in *C. exilis* is about equal to the length of the sepals and hypanthium combined; the ovary of *C. unguiculata*, on the other hand, is conspicuously shorter than the combined length of the sepals and hypanthium.

A study, by Vasek, of the morphological and cytogenetical relationship of these two species is still in progress.

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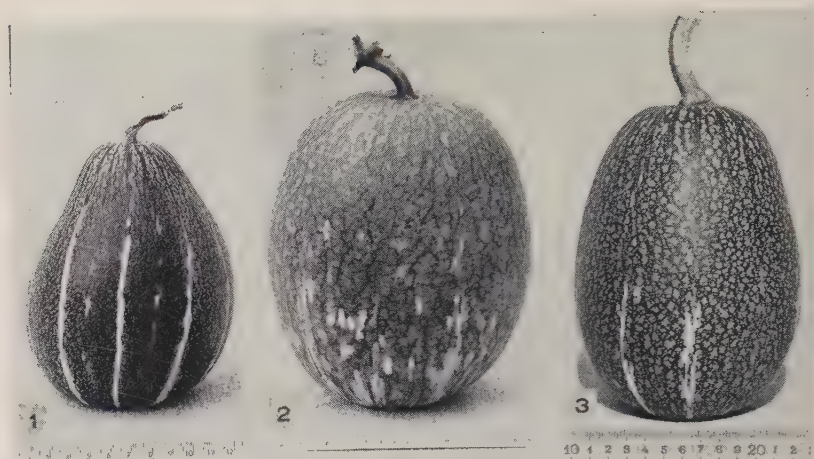
## A CROSS BETWEEN AN ANNUAL SPECIES AND A PERENNIAL SPECIES OF CUCURBITA

THOMAS W. WHITAKER<sup>1</sup>

Except for *Cucurbita ficifolia* Bouché, a perennial, the species of cultivated *Cucurbita* are annuals (*C. pepo* L., *C. maxima* Duch., *C. mixta* Pang., and *C. moschata* Duch.). Repeated attempts to hybridize *C. ficifolia* with the four annual species have been failures, although occasionally small parthenocarpic fruits are set when pollen of *C. ficifolia* is used on female flowers of *C. pepo*. These results have been interpreted to mean that the sterility barriers between *C. ficifolia* and the four annual species are normally sufficient to prevent a flow of genes in either direction.

The perennial habit is of considerable interest in this group, appearing to be an adaptation to xerophytic conditions, and it becomes increasingly significant in relation to the northward spread of members of the group from tropical and subtropical Central America and southern Mexico to the deserts of northern Mexico and southwestern United States. In

<sup>1</sup> Aided by a grant from The American Academy of Arts and Sciences. My thanks are due to G. A. Sanderson, Agricultural Aid, for preparation of the illustrations.



FIGS. 1-3. Mature fruits. FIG. 1. *Cucurbita andreana*. FIG. 2. *Cucurbita ficifolia*. FIG. 3.  $F_1$  (*C. andreana*  $\times$  *C. ficifolia*).

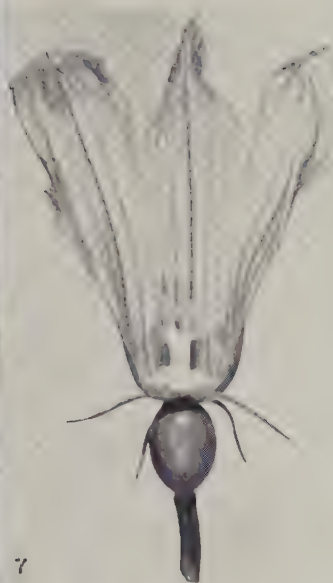
*Cucurbita* it is important to learn something about the genetics of the perennial habit, as it may help in understanding the intricate relationships between the cultivated and the wild species.

In a further attempt to shed light upon the relationship of *Cucurbita ficifolia* to the cultivated annual species, *C. ficifolia* was mated with *C. andreana* Naud., an annual which is evidently the wild prototype of *C. maxima* (Whitaker, 1951). When pollen of *C. ficifolia* was placed on pistillate flowers of *C. andreana*, three fruits were obtained from a dozen pollinations. The results of genetic and cytological observations on the behavior of these hybrid plants are recorded herein. In earlier attempts to make this cross, fruits were obtained, but the seeds were flat and mostly empty or contained small aborted embryos.

#### METHODS AND RESULTS

*Cucurbita andreana* and *C. ficifolia* were described in previous publications (Whitaker, 1951; Whitaker and Bohn, 1950). The fruit of *C. andreana* is an oval, medium-sized, hard-shelled, bitter-fleshed gourd with mottled dark green skin (fig. 1). The vine habit is running rather than bush; typically, every runner produces roots at the node. Fruits of *C. ficifolia* are large and usually have a mottled light green skin (fig. 2). The flesh is white and stringy but not unpleasant to the taste. The variety used in these experiments had large, black seeds with light margins. The plants produce strong, vigorous runners, which may or may not root at each node.

Of the three fruits obtained from twelve pollinations of *C. andreana*  $\times$  *C. ficifolia*, one was normal in appearance, and the others were small and under-developed. Each fruit contained numerous seeds, the majority of



FIGS. 4-6. Mature leaves. FIG. 4. *Cucurbita andreana*,  $\times 2/5$ . FIG. 5. *Cucurbita ficifolia*,  $\times 1/2$ . FIG. 6.  $F_1$  (*C. andreana*  $\times$  *C. ficifolia*),  $\times 1/3$ . FIGS. 7, 8. Flowers of  $F_1$  (*C. andreana*  $\times$  *C. ficifolia*). FIG. 7. Pistillate flower,  $\times 1/2$ . FIG. 8. Staminate flower,  $\times 1/2$ . Note short staminal column and lack of pollen.

them flat. When the seeds were dissected, the embryos were found to be small and poorly developed or completely absent.

When planted, seeds from the smallest fruit failed to emerge. From the other two fruits about one dozen plants emerged, of which six were albino-lethals, and did not produce true leaves before they died. Several of the green plants had cotyledons with white sectors. However, the first true leaves were normal green, and thereafter the plants were extremely vigorous.  $F_1$  plants were transplanted and observed in the field during two successive years. Their behavior was similar each year.

As noted above the  $F_1$  plants when transplanted to the field were extremely vigorous, with runners up to fifty-five feet in length. The runners root at each node, resembling *C. andreana* in this respect. In cross-section, they are 5-angled, but not sharply so as in *C. ficifolia*. In contrast *C. andreana* has runners that are round in cross-section. The hybrid appeared to be an annual under our conditions.

The leaves of the hybrid (fig. 6) are intermediate in appearance between those of the parents (figs. 4 and 5). The setae of the leaves, petioles and stems are more or less soft and flexible, much like those of *C. ficifolia*; those of *C. andreana* are harsh and stiff to the touch.

Staminate flowers usually abort prior to anthesis, but an occasional flower may open in normal fashion (fig. 8). Pistillate flowers were produced at irregular intervals, but not in great abundance (fig. 7). Fruits set readily when the flowers are open pollinated or backcrossed to either parent. However, all fruits from these matings and the open pollinated fruits were parthenocarpic. More than fifty fruits have been examined. The seeds were small, flat and under-developed. When dissected there was no evidence of an embryo within the seedcoats. Apparently almost any kind of pollen furnishes sufficient stimulation to cause fruit development, but apparently no fertilization takes place. The mature fruits were intermediate in size between those of the two parents (fig. 3), but they had a hard shell and bitter flesh similar to those of *C. andreana*. The peduncle was sharply 5-angled and slightly flared at the fruit attachment. Except for size, the exterior appearance of the fruit is much like that of *C. ficifolia*.

Since the staminate flowers of the  $F_1$  plants almost always deteriorate prior to anthesis, pollen counts of opened flowers were not obtained. However, pollen grains from unopened flowers were studied. In aceto-carminic smears of more than ten flowers, no stainable pollen grains were observed.

In comparable smears of anthers from both parents, 97 per cent stainable pollen was recorded for each.

The cytological picture in the  $F_1$  hybrid was one of great irregularity. Of the twenty chromosomes contributed by each parent, not more than ten were paired. The unpaired chromosomes migrate precociously to the poles, thus producing metaphase plates with many irregularities. Some of the unpaired chromosomes are lost in the cytoplasm, and at M II, plates

with eighteen to twenty-two pairs of chromosomes were frequently observed. The irregularities at meiosis probably account for complete sterility of pistillate and staminate flowers.

### CONCLUSIONS

It may be concluded from the observations recorded above that:

1. *Cucurbita ficifolia* is genetically well separated from the cultivated annual species. Its closest connection is probably with *C. maxima* through *C. andreana*, the wild prototype of this species. However, the sterility of the  $F_1$  hybrid between *C. andreana*  $\times$  *C. ficifolia* is so complete that an exchange of genes is stopped in the initial stages.

2. The  $F_1$  hybrids were extremely vigorous after the first true leaves appeared. Usually only pistillate flowers reached anthesis in the  $F_1$  plants. They were sterile when backcrossed to either parent. Parthenocarpic fruits were produced in great abundance by the  $F_1$  plants. The great vigor of the  $F_1$  plants can be accounted for in part by the fact that the vines produce no seed, but this explanation may be only partial. It is likely that hybrid vigor may have made a considerable contribution to the unusual growth of these plants.

3. For the most part the characters contributed by *Cucurbita ficifolia* were dominant in the hybrid, e.g., fruit shape, skin color; angled stem and petiole; more or less soft, pliable setae. The following *C. andreana* characters were evident: hard shell, bitter flesh, and annual growth habit.

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## A NEW SPECIES OF BOWLESIA FROM PAKISTAN

EUGENE NASIR

The genus *Bowlesia* consists of three rather well-defined groups of species which have been accorded different status by different authors. The first group consists of those Chilean plants with erect, dichotomously branched stems and with stipules reduced to discrete cilia. These Bentham (1867) referred to his section *Homalocarpus* while Drude (1897) to his section *Elatae*. The second and third groups comprise weak-stemmed, prostrate, or spreading plants with evident, usually lacerate or fimbriate stipules; the second group has fruits bearing essentially sessile stellate hairs but no glochidia, whereas the third group has separate glochidia or, more commonly, a glochidiate wing extending along the

lateral ribs and sometimes also along the dorsal rib. Bentham regarded the presence of glochidia as of specific rather than generic value, and included the two latter groups in his section *Eubowlesia*. Drude thought it of generic significance, placing the non-glochidiate fruited species in his section *Tenerae* of *Bowlesia*, but referring the glochidiate ones to the allied genus *Drusa* of the Canary Islands and northern Africa. *Drusa*, according to Bentham, was also to be regarded as part of *Bowlesia* section *Eubowlesia*. Most subsequent authors have apparently referred all American material to *Bowlesia*, and have restricted *Drusa* to the type species, *Drusa glandulosa* (Poir.) H. Wolff or *D. oppositifolia* DC., of the Old World.

An understanding of these interpretations of *Bowlesia* and *Drusa* is important here because any conclusions as to the geographical patterns of distribution depend upon how many genera are recognized, and to which of the two the glochidiate-fruited American species are assigned.

Excluding section *Homalocarpus* or *Elatae* of *Bowlesia*, some of the more important characters of *Drusa* and the remaining species of *Bowlesia* may be tabulated as follows:

<i>Bowlesia</i>	<i>Bowlesia</i>	<i>Drusa</i>
(non-glochidiate species)	(glochidiate species)	Calyx obsolete
Calyx lobes evident	Calyx lobes evident	Fruit with lateral ribs
Fruit stellate-pubescent, non-glochidiate, inflated	Fruit with lateral and dorsal ribs glochidiate,	glochidiate-winged, otherwise glabrous,
Stem stellate-pubescent and sometimes with simple hairs	often stellate-pubescent, angled or variously flattened	dorsally flattened
	Stem stellate-pubescent and sometimes with simple hairs	Stem glochidiate, otherwise glabrous

It is evident from the comparative study of the non-glochidiate and glochidiate species of *Bowlesia* and that of *Drusa* that there is a complete bridging of the morphological gap between the two genera, and it is difficult to tell where one genus ends and the other begins. Taking all these facts into consideration, it may be advisable to include *Drusa* within *Bowlesia*.

*Bowlesia* is chiefly a South American genus, extending from southern Chile and Argentina northward to Ecuador and Brazil, and recurring in Central America and Mexico, with one species in Texas and California. *Drusa* is confined to the Canary Islands and Morocco. Thus neither *Drusa* nor *Bowlesia* has been reported from either Europe or Asia, so the discovery of a species of *Bowlesia* in northwestern Pakistan is truly a remarkable find.

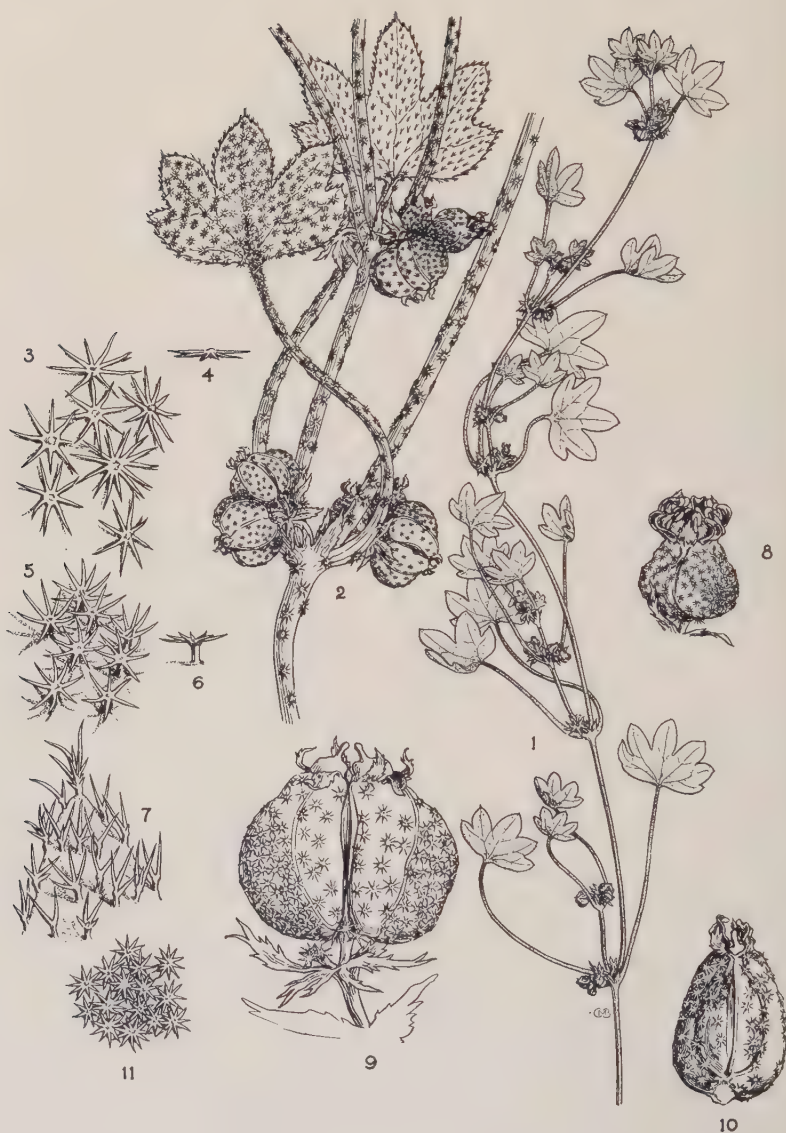
This plant was collected in the first week of May, 1936, by Ramchand, a student of Gordon College, in Topi Park, two or three miles from Rawal-

pindi. Rawalpindi is situated between  $33^{\circ}$  and  $34^{\circ}$  N. latitude at an altitude of 1600 feet, on a plain about 15 miles south of the foot of the Himalayas. Topi Park occupies a slightly hilly area of about four square miles, which has three ponds and a small ravine that remains dry for half the year. The vegetation of this area is preserved in its natural state and consists mainly of such xerophytic plants as *Acacia modesta*, *Gymnosporia Royleana*, *Otostegia limbata*, *Olea cuspidata*, *Adhatoda Vasica*, *Zizyphus nummularia*, *Ehretia aspera*, *Tribulus terrestris*, *Filago germanica*, *Cousinia minuta*, and many others. Near water and in low-lying areas where the soil remains damp for a longer period, there are many herbs, such as *Ranunculus arvensis*, *R. muricatus*, *Sisymbrium Irio*, *S. Sophia*, *Senebiera didyma*, *Lepidium Draba*, *Geranium rotundifolium*, *Medicago denticulata*, and *Rumex dentatus*.

It is probable that the specimen of *Bowlesia* was collected in one of these moister and more sheltered spots. There are doubtless specimens of this species extant in the larger herbaria of the world, but their identity has not been recognized.

***Bowlesia asiatica* sp. nov.** Herba annua prostrata dichotoma caulibus gracilibus quadrangulosis elongatis basi diametro  $\frac{1}{2}$ – $\frac{3}{4}$  mm. striatis glabratis vel pilis 8-radiatis breviter stipitatis vel sessilibus leviter stellato-pubescentibus obsitis. Folia opposita 10 mm. longa, 15 mm. lata petiolis longis connatis laminis lobis 5 (4–6) tribus centralibus inter se similibus ovalibus vel ovatis eis 2 lateralibus minoribus palmato-lobatis subtus albo-griseis basi venis principalibus 3 (interdum 4–5) valde ventais ambitu pilis stellatis 8-radiatis stipitatis praecipue subtus obsitis pilis inaequaliter radiatis praesertim supra ad apices versus obsitis marginibus pilis 2–5-radiatis item stellato-pubescentibus. Petioli laminibus 4–6-plo longiores stipulis lanceolatis albis membranaceis minute laceratis instructi. Umbellae simplices axillares subsessiles  $\pm$  2–5-floribus ex nodis erumpentes. Involucri bractae paucae albae scariosae laceratae. Flores parvi calycis dentibus prominentibus lanceolato-ovatis albis laceratis petalis purpureis late ovalibus acutis inflexis calycis dentes paulo superantibus stylo brevi stylopodio parvo conicali carpellis juvenibus dorsaliter paulo compressis dense stellato-pubescentibus. Fructus ellipsoideo-globosus subsessilis pilis stellatis eis caulis dimidio longioribus obtectis jugis inconspicuis faciebus dorsalibus inflatis quam faciebus noninflatis lateralibus magis pubescentibus usque ad 2 mm. longus 1.5 mm. latus.

Prostrate, dichotomously branched annual (fig. 1), the stem slender, quadrangular and  $\frac{1}{2}$ – $\frac{3}{4}$  mm. in diameter near base, up to 20 cm. long, grooved, glabrate to lightly stellate-pubescent, the hairs 8-rayed with short stalks or sessile (figs. 3 and 4); leaves opposite, long-petiolate, leaf lamina 10 mm. long and 15 mm. broad, palmately lobed, usually with 5 (4–6) lobes, the 3 central lobes equal, oval to ovate, the 2 lateral lobes smaller (fig. 2), prominently veined on the ventral surface, with usually 3 (occasionally 4 or 5) principal veins at base, the ventral surface whitish-



FIGS. 1-11. *Bowlesia asiatica*. FIG. 1. Habit sketch,  $\times \frac{2}{3}$ . FIG. 2. Habit sketch,  $\times 3$ . FIGS. 3, 4. Stem hairs,  $\times 20$ . FIGS. 5, 6. Dorsal leaf hairs,  $\times 20$ . FIG. 7. Distal leaf hairs,  $\times 20$ . FIG. 8. Flower and young carpel,  $\times 10$ . FIG. 9. Mature fruit, lateral view,  $\times 10$ . FIG. 10. Single carpel, commissural view,  $\times 10$ . FIG. 11. Fruit hairs,  $\times 20$ .

grey, both surfaces covered with stalked, stellate, 8-rayed hairs (figs. 5 and 6), these more numerous on the ventral surface, unequally 2-5-rayed hairs occurring on the upper surface especially distally, the margins also stellate-pubescent with 2-5-rayed hairs (fig. 7); petioles 4-6 times longer than the lamina, connate at base, stipulate, the stipules lanceolate, white, membranous, minutely lacerate; umbels simple, axillary, borne at the nodes, subsessile, 2-5-flowered (fig. 2); involucre of a few scarious, white, lacerate bracts; flowers small; calyx teeth prominent, lanceolate-ovate, white, lacerate; petals purple, broadly oval, acute, inflexed, slightly longer than the sepals; style short; stylopodium small, conical; young carpels slightly compressed dorsally, densely stellate pubescent (fig. 8); fruit ellipsoid-globose, subsessile, up to 2 mm. long and 1.5 mm. broad (figs. 9 and 10), covered with stellate hairs half as large as those of the stem (fig. 11), the ribs inconspicuous, the inflated dorsal sides hairier than the uninflated laterals.

Type. Topi Park, Rawalpindi, Pakistan, May, 1936 *Ramchand* (herbarium of Gordon College, Rawalpindi).

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#### REVIEWS

*New Zealand Pollen Studies, The Monocotyledons.* By LUCY M. CRANWELL. 91 pp., 8 collotype plates, 66 text figures, 1 map. 1953. Bulletin of the Auckland Institute and Museum, No. 3. Published by Harvard University Press for Auckland Institute and Museum. Cloth, \$5.00; paper, \$3.50.

Lucy M. Cranwell's recent account of the pollen of the New Zealand monocotyledons is a welcome addition to the expanding volume of literature dealing with the comparative morphology and taxonomy of pollen of the world's vegetation. Gilbert Archey, Director of the Auckland Institute and Museum in his introduction to the present bulletin has correctly pointed out the importance of the need for a sound morphological and systematic basis in dealing with pollen of living plants before proceeding with identification and interpretation of fossil pollen, as well as for the overall application of pollen studies as an increasingly important biosystematic discipline in taxonomy. In addition, applied fields, such as hayfever and honey investigations, should ultimately benefit from these studies. It is, however, primarily to the object of

"interpretative study of past vegetation- and climate-change" based on fossil pollen sequences that the present morphological study is dedicated.

Before proceeding with a detailed discussion of the morphology and systematics of the New Zealand monocotyledonous pollen types, Miss Cranwell considers briefly the nature and uses of monocotyledonous pollen, economic as well as scientific, and in a general discussion introduces topics on the basic morphological types of grains found among monocotyledonous families, the range of color displayed by the exine of various grains, the main methods of pollination and the apparently attendant morphological modifications which occur in conjunction with these methods. In addition the methodology used in preparing and studying the material discussed in the present work is included.

A glossary of pollen terms is included, which draws most heavily on the morphological terminology proposed in the publications of Potonie, Wodehouse, Erdtman, Selling, Faegri and Iversen, and Iversen and Troels-Smith, all veteran pollen workers. Diagrams which would aid the layman in understanding many of the terms used by palynologists have unfortunately not been included by Cranwell nor by a good many of the others who have proposed the terminology presently employed, with the notable exception of the excellent illustrations found in the publications of Faegri and Iversen and Iversen and Troels-Smith. A number of good photomicrographs, however, illustrating the main classes into which pollen grains fall, are appendaged. These classes are based on the occurrence of the grains in coherent groups or singly, and on the type and arrangement of apertures. A key to the families and in some cases genera and species of monocots found in the New Zealand flora is included. Photomicrographs or drawings illustrate many of these.

The bulk of the bulletin is devoted to a systematic treatment of pollen types found among the 11 orders and 17 families of New Zealand monocots, none of which are endemic to that country. The main approach followed here is a survey of the views regarding the taxonomy of the orders, families and genera as treated by various systematists and pollen morphologists, a detailed comparative morphological examination of the pollen as studied by Miss Cranwell, including references to other publications illustrating and dealing with the morphology of any of this material, some data on the physiognomic habit, ecology and distribution of the plants in New Zealand, the manner of pollination and the relative amounts of pollen produced, and information on fossil occurrences of members of any of these orders, either in New Zealand, or as recorded elsewhere by other pollen workers. In her systematic treatment, Cranwell follows the family classification very much as given by Skottsberg, whom she points out was considerably influenced in his views by Wettstein, and in her arrangement of minor groups the classification of Engler and Diels or Krause. However, she points out that "attention is given to Hutchinson's views throughout, since many of his innovations, while revolutionary, are pollen-morphologically attractive . . ." She further notes that all systems offer difficulties when dealing with the pollen and she for the most part leaves for later sifting by "pollen-conscious systematists" the data derived from a consideration of the pollen morphology.

A table at the end of the systematic account sums up the salient characters (aperture, exine sculpture, size) of the pollen of species of New Zealand monocots.

Miss Cranwell's efforts should be well received by systematists and palynologists alike, for the publication includes a great deal of material of interest to scientists working in both of these disciplines. Not only does she contribute a great deal of original material to our knowledge of the morphology of monocotyledonous pollen grains, but throughout the work she has excellently supplemented all her discussions with references from the vast literature dealing in all ways with pollen morphology. While the results of her work are most immediately applicable to palynological investigations, it is to be hoped that they will inspire more taxonomists as well as palynologists to interest themselves in the possibilities which comparative pollen morphology offers as a biosystematic approach to the problems of plant classification, and to the possibilities of establishing more natural relationships within the classificatory scheme.

—JANE GRAY, Department of Paleontology, University of California, Berkeley.

*Flora of Bedfordshire*. By JOHN G. DONY. 532 pp., 22 figs., and 24 plates. 1953. The Corporation of Luton Museum and Art Gallery, Wardown Park, Luton, Great Britain. 42s. net.

Bedfordshire is a small county in the South Midlands of England. It is only 35 miles long and 25 miles wide at its greatest breadth and has a population of about 312,000. It was one of the first counties in England to have a published Flora (1798). The history of botanical investigation in this area is largely an interesting story of the accomplishments of enthusiastic amateurs. The present book, culminating an 18-year study, is the work of the volunteer Keeper of Botany at the Luton Museum. It was published by the Luton Museum with the support of the Luton Borough Council and the Royal Botanical Society. The assistance of the Borough Council in obtaining subscriptions from Bedfordshire residents and botanical organizations the world over is especially interesting. How many American city or county commissions would be willing to support work of this type?

"Habitat studies" of nearly a hundred selected localities are a noteworthy feature of this flora. Exact location, estimated relative abundance of species, soil type, and pH were noted at each site. Numerous photographs illustrate the habitat study areas. These detailed stand data will be of value in noting change in frequency and abundance of species within the county in future years. The author unfortunately merely lists these analyses according to "natural areas"; thus he does not succeed in effectively organizing the information so that a picture of community types emerges for the reader.

A list of flowering plants, ferns and fern allies, mosses, liverworts, stoneworts, and fungi comprises the bulk of the book. Both scientific and common names, with synonyms from standard manuals of the British Isles, are given. Records for each species are listed in historical sequence according to botanical districts. Habitats occupied and distribution in neighboring counties are presented. To an American the omission of keys seems to limit the usefulness of the flora. It must be considered, however, that the flora of the entire British Isles is relatively small, and excellent keys are readily available for the flowering plants and ferns. Keys for the lower plants, on the other hand, would have been of considerable value as manuals covering these are difficult to obtain.

An extensive bibliography of published material, relating to the flora of neighboring counties as well as Bedfordshire and a list of herbaria containing specimens from the county are included. There are separate indices not only of plant names but also of botanists and place names.

Obviously this book has required an enormous amount of meticulous labor. Such elaborate cataloging of details regarding a small area is somewhat astounding to an American. Because of the areal extent of our country and the size of our flora we have not as yet attempted such compilations. It also is perhaps surprising to us that this work has been executed primarily by individuals enthusiastically interested in, although not professionally occupied with botany. Such a publication is of great service in providing local information which may be utilized to advantage by the professional botanist doing comprehensive and interpretative studies. JEAN LANGENHEIM, Department of Botany, University of California, Berkeley.

## NOTES AND NEWS

PRESENCE OF ALPINE PLANTS ON LASSEN PEAK, CALIFORNIA. During the summers of 1952 and 1953 while working as a ranger-naturalist in Lassen Volcanic National Park this writer carefully examined the upper reaches of Lassen Peak, which rise to an altitude of 10,453 feet, and found that its alpine plants are currently represented by substantial populations. Certain of these species, namely *Draba aureola* Wats., *Colomia larsenii* (Gray) Payson, and *Smelowskia ovalis* Jones var. *congesta* Rollins have interesting patterns of distribution in that they are largely restricted to certain peaks of the Cascades. The *Smelowskia* is endemic to Lassen Peak, while the others

have never been found on Mount Shasta nor on any of the mountains between Lassen Peak and central Oregon. These rare alpine plants were discovered on Lassen Peak by J. G. Lemmon (1874). Limited collections were made by Mrs. R. M. Austin (1882) and Chesnut and Drew (1889). The failure of botanists to note these plants since the early collections were made probably stems from the misinterpretation of the recent volcanic activity on Lassen Peak. In comparatively recent botanical literature (Drury & Rollins, *Rhodora* 54:108, 1952; Jepson, *Fl. Calif.* 3:172, 1943) the 1914-17 activity has been credited with the extinction of flowering plants on the upper levels of the peak by such references as "explosive disappearance of the upper portion of the peak," and "the apparent destruction of alpine and sub-alpine herbs."

It is of interest to note that in treating *Collomia larsenii*, Jepson (*loc. cit.*) states that on his visit to Lassen Peak in September, 1929 "... the mountain top was deeply covered with ash and mud from the volcanic eruptions and no herbaceous vegetation was found above the 9300 foot level." Jepson unfortunately selected a route which to this day is largely devoid of plants on its upper reaches. His meticulous and well-written field notes reveal that he approached Lassen Peak from the west via Manzanita Creek and ascended the western slopes. This route is characterized by mud flows and was devastated by an extensive mud flow in May, 1915.

From documented accounts of the 1914-1917 volcanic activity on Lassen Peak it is quite obvious that many popular ideas of what happened are, at best, gross exaggerations of the truth. The lava which was extruded in May, 1915, though covering most of the summit, did not extend to the south and south-east portions. The great horizontal blast which followed three days later traveled over the northeast sector of the peak. Though of sufficient intensity to fell large trees more than three miles from the crater, the blast by no means succeeded in blowing off the top of the peak. This point is readily substantiated by the still visible anchorages used to secure a lookout house built on the summit prior to 1914. Most of the ejecta of the numerous lesser explosions was fine material, and prevailing winds carried the majority of it to the north and northeast slopes. Since then, mud flows have coursed down all slopes of the peak, though these can generally be said to have been less frequent on the south slopes, which at the higher levels are less steep than those of the other exposures. These factors are largely responsible for the present existence of the alpine plants of Lassen Peak on the south slopes between an elevation of about 9400 feet and the summit. GEORGE W. GILLET, Department of Botany, University of California, Berkeley.

Milo S. Baker, Professor of Botany, Emeritus, of Santa Rosa Junior College, has recently completed a greatly augmented "Partial List of Seed Plants of the North Coast Ranges of California" (June, 1954, mimeographed) in which 2435 plants are listed, a total of 318 more than were included in his 1951 Check List. Those wishing copies may obtain them by sending fifteen cents in postage to Milo Baker, Santa Rosa Junior College, Santa Rosa, California.

The "Manual of Southwestern Desert Trees and Shrubs" by Lyman Benson and Robert A. Darrow, which has been out of print since 1947, is again available, as a second edition, under the slightly altered title, "The Trees and Shrubs of the Southwestern Deserts" (University of New Mexico Press, Albuquerque, New Mexico, \$8.50). Relatively few changes were made in the text, but improved typography will make this second edition much more enjoyable to use. The book is written in non-technical language. In addition to keys and descriptions, it contains interesting and useful notes on many of the plants, as well as distribution maps and an abundance of excellent illustrations in color, half-tone and line.